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ferrets, etc., and to destroy only the does taken, setting the males free. "The results of this mode of operation are that the male rabbits, as soon as they begin to predominate in numbers, persecute the females with their attentions, and prevent them from breeding. They also kill the young rabbits that happen to be born, and where they largely predominate in numbers, worry the remaining does to death." Mr. Rodier states that on his station an eight months' trial has resulted in clearing the country of the pests.

Fishes.—Jenkins and Evermann describe (Proc. U. S. Nat. Mus., 1888) eighteen new species of fishes from Guaymas, on the Gulf of California. Hermosilla is a new genus of the family Sparidæ, and Pseuddoblennius of the family Blenniidæ. Clevelandia is reduced to synonymy as was done some time ago in this Journal.

Dr. R. W. Shufeldt (*Jour. Morphology*, II., pt. 2) has given some details of the osteology of an unique specimen of the fish *Grammicolepis brachiusculus* Poey. The account is too detailed for abstract, but with Professor Poey, Dr. Shufeldt thinks the relationships of the Grammolepidæ tend mostly in the direction of the Casangidæ.

Birds.—Mr. F. E. Beddard concludes (*Proc. Zoöl. Soc.*, London), from a study of the muscular system of Polyboroidei, that this genus is not even an aberrant type of Falconidæ, and does not deserve even sub-family rank.

EMBRYOLOGY.

A Physiological Hypothesis of Heredity and Variation.—The extravagant claims made by Prof. Weismann, the author of the doctrine of the *isolation of the germ-plasma*, and of the doctrine of *heredity* based upon it, as well as the strenuousness with which it is insisted that there is no other way in which the facts of inheritance may be coördinated, requires that a re-examination be made of the grounds upon which those claims are supposed to rest. This is all the more necessary, in that this author and his followers repudiate the evidence upon which the claim is made that acquired characters, taken in the widest Lamarckian sense, can be transmitted. During a period extending over fifteen years the present writer has devoted himself to a study of the genesis of adaptations, and with the lapse of time the conviction has grown only the clearer that these authors are laboring under a delusion. The way in which they have placed themselves upon record shows that they have not reckoned with the consequences of their reckless speculations.

In the first place, the supposition of a germ-plasma distinct from the plasma of the parent-body is a needless interjection into the machinery of hypothesis of biological evolution. It does not make the matter one whit clearer to suppose that the germ-plasma is necessary, than to suppose that *all of the living plasma of any and every distinct species is an idioplasm, or is specific in so far as that species is concerned.* If we now suppose, as a consequence of the action of the principle of physiological division of labor, first propounded by H. Milne-Edwards, that *all the plasma, or the whole of the specific protoplasm or idioplasm of the organism, becomes physiologically differentiated and incapable of undergoing embryonic development, except that of the germ-cells or germinal plasma,* as long ago urged by Professor Huxley, we get the same result as that reached by Weismann without involving ourselves in the consequences which beset his hypothesis. *This germinal matter is the only functionless and idle plasma in the parent body,* capable of growing and consequently of multiplying its cellular units within the parental organism at the expense of the surplus metabolism of the latter as a whole. Moreover, the germinal cells are alone capable of detaching themselves, or being detached, from the parental organism as products of over-nutrition, which have become useless to the life of the parent, as assumed in my preliminary paper "On the origin and meaning of sex." This recognizes the apparent fact of the setting aside or isolation of the germ-plasma, but does not make that fact the cause of the stability of species through the continuity of processes of growth, and the assumed but not empirically demonstrated isolation of such germ-plasma. My interpretation is in absolute accord with the requirements of the principles of modern physiology, while the hypothesis of Weismann and his followers is in conflict with those principles, and ultimately, as a necessary consequence, with the still more comprehensive principle of the conservation of energy. Modern physiology, as well as the doctrine of the conservation of energy, positively forbids us to interpose any barrier between the plasma of the parent-body and that of the germ-cells, as is done by the promulgators of the hypothesis of the continuity and isolation of the germ-plasma. To do so robs us of the possibility of appealing to the agency of the workings of metabolism as the efficient causes of the modification of the germinal matter. Since metabolism, and all that it implies, is the only agent to which, according to modern physiology, we can appeal, without interjecting gemmules, plastidules, pangens or some other accessory and needless agency into living organisms, as the efficient agents in the transmission of hereditary traits, we are restricted in our choice to metabolism alone. In this way only is it possible to get rid of a *deus ex machina* in the form of an idioplasm in the sense first implied by Nägeli, or of the gemmules of Darwin and Brooks, the plastidules of Haeckel, the pangens of De Vries, or the physiological units of H. Spencer.

The preceding paragraph contains, in essence, my own hypothesis according to which all the facts of hereditary transmission and variation may be coördinated without losing or rendering unavailable the advantages which may be derived from the supposition that acquired characters may be transmitted.

On my view metabolism itself becomes the means of transmitting the changes in the adult organism, due to the complex interaction between it and its surroundings to the idle, functionless and passive germ-cells, because it is a demonstrable fact that these are the only cells in a multicellular organism which have no work to perform which is of direct benefit to the individual life of that organism, unless it may be to take up the surplus nutriment not used up by the metabolism of the parent-body in the secular exhibition of the sum total of its physiological energies, in the struggle for existence.

On my view the idioplasmic or specific molecular character of the plasma of the germ-cells, in common with that of the protoplasm of the whole body (which latter always tends to repair injuries, or even, in lower forms, replace lost parts), tends, in virtue of its acquired specific traits, to repeat the organization of its parent type, in the course of its development, not because it is something different from the protoplasm of the cells of the rest of the body, but because it is wholly unspecialized and without physiological differentiation, as first urged by Prof. Huxley and subsequently maintained by H. Spencer. Molecular impressions experienced in the course of variations in the modes of manifestation of or disturbances of the balance of the metabolism of the parent-body are supposed upon this view to be transmitted as molecular tendencies to the idle or passive plasma of the germ-cells. Variations in the molecular constitution and tendencies of the germinal matter are supposed to thus arise at different times in the same parent, and that, consequently, successive germs may be thus differently impressed. In this way also the molecular tendencies of the plasma of the germ-cells of different individuals may be also modified simultaneously or successively through the effect of enforced changes in the metabolism of multitudes of contemporaneons adult individuals of the same species, thus leading to a tendency toward concurrent or simultaneous variation of offspring in the same or a similar direction.

It will be seen that this is the only hypothesis which renders the possibility of concurrent or simultaneous variation within the limits of a species either conceivable or intelligible. It also lends itself to an intelligible comprehension of the phenomena of the correlation of the growth of parts, and it is also the only view which holds out any promise of coördination with the highly ingenious and suggestive hypotheses of Prof. Wilhelm Roux.[1]

It will be at once perceived that my hypothesis of the acquisition of variations and their transmission is the simplest that has yet been offered. It interjects nothing hypothetical into our conception of the physical substratum of living organisms, except the necessarily unknown and unknowable constitution of the molecular factors of metabolism, already assumed by all scientific physiologists, all of whose conceptions of living processes are based upon the theory of metabolism, and thus brought into harmony with the all-inclusive doctrine of the conservation of energy. Growth and development without accompanying metabolism is simply unthinkable. All of the tendencies, capabilities and manifestations of growth in all living organisms are coëxtensive with and the concomitants of metabolism. From this conclusion there is no possible means of escape. To imagine the existence in living bodies of a hypothetical entity for the sole and express purpose of superintending and ordering the sequence and modes of action of the processes of development is, to the mind of one who is imbued with the true scientific spirit, no better than an appeal to "vitality" to explain the sequence and nature of the phenomena of life. Such methods in biological science ought to have received their quietus from Huxley's suggestive and witty comparison of the "vitality" of an organism with the "horologity" of a clock.

That a certain habit of metabolism is inheritable is sufficiently attested by the notorious transmissibility of the tendency to obesity in the human race, since illustrations will immediately occur to almost every one of families in which the tendency is known to be hereditary. Similarly there is scarcely a possibility of doubt that the greatly increased fertility of domesticated races of animals and plants is almost exclusively due to a gradually increased power of appropriating nutriment due to a change in the molecular habit or mode of metabolism of the plasma of the body under better conditions of nourishment which has been slowly augmented and fixed by inheritance.[2] Even certain species of fishes, when well fed and kept in confinement, not only spawn several times during a season, instead of only once, as I am informed by Dr. W. H. Wahl, but also when kept from hibernating, as he suggests, tend to vary in the most astounding manner. The wonderful results of Dr. Wahl, attained in the comparatively short period of six years, show what may be done in intensifying the monstrous variations of Japanese gold-fishes, through selection, confinement in tanks and aquaria, with comparatively limited room for swimming, plenty of food, etc., all of which conditions tend to favor growth and metabolism, and the expenditure of energy under such wholly new and restricted conditions as to render it almost certain, as he thinks, that these factors have something to do with the development of the enormous and abnormally lengthened pectoral, ventral, dorsal, double anal and caudal fins of his stock. Some of the races of these fishes have obviously been affected in appearance by abundant feeding, as is attested by their short, almost globular bodies, protuberant abdomens, and greedy habits, as I have observed in watching examples of this short-bodied race living in Dr. Wahl's aquaria. In these last instances we are brought face to face with modifications occurring in fishes under domestication which are infinitely in excess, morphologically speaking, of anything known amongst any other domesticated animals. That the abundant feeding and exposure to a uniform temperature during the whole year, and confinement in comparatively restricted quarters, has had something to do with the genesis of these variations, through an influence thus extended upon the metabolism affecting the growth of certain parts of the body, which have tended to become hereditary, there can scarcely be any doubt.[3]

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[1] Der Kampf der Theile im Organismus. 8vo., pp. VII., 244, Leipzig, 1881.

That such changed conditions would also favor variability to a high degree we cannot doubt. That, moreover, the passive or idle plasma of the germ-cells would be thus impressed by changes in the manner of exhibition of the metabolism of the bodies of the parent fishes there can scarcely be any question.

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[2] The only possible explanation of the phenomenon of the after-effect of the first impregnation of the viviparous female mammal upon all subsequent offspring by the male parent of such first offspring can be explained only upon the supposition that this singular effect is produced, as supposed by Sir W. Turner, through a permanent disturbance of the metabolic habit of the maternal organism by that offspring during the first uterine gestation.

[3] Since the foregoing was written, I have been able, through the helpful generosity of my friends, Mr. W. P. Seal and Dr. Wahl, who have supplied me with fresh material, to verify my suspicion that the muscular system of the highly modified races of Japanese gold-fishes is relatively much less well developed than in the usual and unmodified type found in open rivers. From careful dissections and measurements the evidence is conclusive that the muscular system has undergone marked degeneration and enfeeblement in the extremely modified domestic races, probably owing to disuse.

In fact, if it is borne in mind that the extremely unspecialized and functionless condition of the germinal plasma is in itself favorable to variation through its impressibility by imperceptible changes in external conditions, we should almost cease to wonder at the variability of multicellular animals, which, as is well known, is always intensified under the influence of domestication.

It may not be generally known that abundant food and inappreciable variations of the conditions of life exert a most astonishing influence upon the size, form, rate of multiplication, and tendency toward conjugation in ciliated infusoria. The individuals differ in size by many times the bulk of the smallest condition of the same species, and there are equally great and unaccountable differences in form arising from unknown causes, as I learn by keeping the same colonies under prolonged observation, and no less than two very distinct and highly characteristic modes of fission may occur in the same species, whether free or attached in habit. So great are these differences that I am convinced that individuals of one and the same species have been regarded in some instances as distinct species by different observers who have not observed the same form under a great variety of conditions.

In an earlier essay,[4] giving synoptically the results of an extended study of the subject of sex, I assumed that the egg, or oösperm, was inherently more capable of variation in its early stages of development than during the later larval or adolescent period. This is what should have been expected if my hypothesis of the nature of heredity and the causes of variation is true, no less than upon the ground of the known want of morphological specialization characterizing all the germs of multicellular organisms. If we seek for facts in support of this view, we have them in abundance in the extreme sensitiveness of the ova of many metazoa to outward influences of the most trivial character. I may cite in illustration the well-known experiments of Weber in producing monstrosities from the recently fertilized eggs of the pike by simply shaking them somewhat roughly. To the same effect speaks the fact, well-known to fish-culturists, that the eggs of Salmonidæ immediately after fertilization must be handled with extreme care, some experienced persons declaring that it is even dangerous to disturb them in any way for the first few days, lest the

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[4] Origin and Meaning of Sex. AMER. NATURALIST, pp. 501-508, Vol. XXIII., 1889.

greater part develop as double and variously deformed monsters. Such monstrous and worthless broods of larval salmon, which were doubtless the result of careless or ignorant treatment of the ova, I have myself seen. The same or similar facts are known to intelligent breeders of domesticated birds and fowls. And to this evidence it may be added that such monstrosities occur during development amongst invertebrates in a state of nature, as I have repeatedly observed in the case of the American lobster. The liability to deformities so produced is also known to diminish as development advances, thus firmly establishing, on the basis of fact, the view for which I contend. This also supports my conclusion, previously noted elsewhere, that the divergence of species must be studied from the stand-point that the tendency toward variation and divergence is most pronounced in the egg, and not in the adult, and in response to adaptive requirements to which the adult organism cannot so directly respond. This view further involves the conclusion that widely distinct forms, perhaps even phyla, have been directly evolved from the morula and planula stages as consequences of the greater capacity for direct adaptation possessed by germs in these stages, and that the earlier phases of segmentation are consequently far more significant than the later and more highly modified larval stages, all of which must be regarded as more or less directly adaptive, as a study of their structures and metamorphoses in relation to their surroundings renders self-evident; since many of them possess features which cannot be shown to have ever been of the least use to any conceivable ancestral form of the adult, as, for example, the placenta, amnion, and allantois, which are striking illustrations of this truth.

The father of modern transformism, Lamarck, was also one of the first to appreciate the significance of that foundation principle of modern physiology, which was named metabolism by Theodore Schwann. The further consequences of the differentiation of metabolic processes, *pari passu* with morphological differentiation were traced and elaborated as we have already seen by two great masters in biology, namely H. Milne-Edwards and Huxley, and it has been sought to show that a further consideration of the capabilities of metabolism indicates that it may become the foundation of an intelligible hypothesis of heredity, which takes as its logically necessary basis the assumption that there is no scientific warrant for the belief in the isolation of the germinal matter of living bodies in such wise that it is out of the range of the influence of the effects of the physiological activity of the whole parent organism. This must be so on the ground of the universality of the occurrence of metabolism, even during the maturation or or growth of germs in the parent body, the contrary opinion being in conflict with fact.

The Lamarckian philosophy of transformism therefore offers the foregoing hypothesis of heredity as a substitute for the preposterous one of the isolation of germ-plasma, which, as here shown, is in the most obvious conflict with the principle of the conservation of energy. An isolated germ-plasma is as undemonstrable as the presence of bow-legged goblins in the moon. The primary postulate of that hypothesis is suicidal. There is no middle course to be taken. Biologists who commit themselves to an acceptance of the biological vagaries of Weismann array themselves against the modern rigorously scientific tendency to examine the problems of biology from the standpoint of the physicist.

In that the doctrine of the isolation of the germ-plasma is in irreconcilable conflict with the great cardinal principle upon which the whole fabric of modern physiological science rears its stately proportions, namely, with the general theory of metabolism, and, consequently, with the still more imposing and universal principle of the conservation of energy, we therefore realize what a colossal fabric of speculative rubbish must be consigned to the limbo of untenable and forgotten hypotheses in what is represented by the misguided labors af the advocates of the existence of an unalterable germ-plasma.—JOHN A. RYDER.

PHYSIOLOGY.

An Experimental Investigation of Strychnine Poisoning.—It is a known fact that certain organs extract certain substances from the blood. The kidney, for instance, takes up urea; the motor nerves have an elective affinity for curare, the nervous system for lead, etc. As regards any drug which particularly affects any organ, the question may arise whether the organ is affected because of a special attraction for the drug or because of a special susceptibility to its action. For example, does strychnine affect the spinal cord because the latter stores up a relatively larger quantity of it than other organs, or is the spinal cord more susceptible to its influence than is muscle, or liver, for instance? In order to throw light upon this subject, Dr. Lovett[1] has

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[1] *Journal of Physiology*, Vol. IX., p. 99.